Some Economic Aspects of Taxonomy*

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One dictionary definition of taxonomy is: "Classification; especially classification of animals and plants according to their natural relationships; also the laws and principles of such classification." Another, a bit longer is: "The laws and principles of taxology, or their application to the classifying of objects of natural history; that department of science which treats of classification; the practice of classification according to certain principles." And in this same dictionary taxology, a term I have never wittingly used, and which I shall eschew, is defined as: "The science of arrangement or classification; what is known of taxonomy." Here I infer that the lexicographer responsible for the definition of both taxonomy and taxology may have preferred the latter to the former, but taxonomy, widely and universally used, will scarcely be replaced by taxology, no matter what a lexicographer may prefer.

Under the first definition, including the laws and principles of classification, one could wander far afield and become bogged down in discussions of the laws of nomenclature for nomenclature cannot be disassociated with taxonomy, for we must, of necessity, use names for the objects with which we are concerned. However, I have no intention of thus widening the subject to include problems of nomenclature and interpretations of the rules and regulations set up by international botanical congresses to govern the application of names, for such discussion would be endless.

This topic was assigned to me and is, perhaps, not one that I would have chosen voluntarily. Thus I feel relatively little personal responsibility as to just how I may develop the subject, realizing very fully that no two individuals would treat it in a comparable manner. To limit the definition to "classification according to natural relationships" would be unwise, for in practice, while it is fully realized that arrangement according to natural relationships is the objective that is always desirable, this is not always practicable. Often our reference collections are totally inadequate, and we have to do the best that we can with what is available. The result is that not infrequently we are obliged to utilize characters of a more or less obvious nature, and not always those that indicate the closest natural relationships between various groups, whether these be major or minor categories. Again, we may utilize a combination of obvious utilitarian characters associated with others that clearly indicate natural affinities, in order to attain a certain objective.

As long as the learned world of the early European civilizations up to and

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including the middle ages knew and utilized only a few hundred basic plant species, botanical science and taxonomy was indeed a simple matter. In those distant days a rough classification, as to major groups, as trees, shrubs, and herbs sufficed. Species were designated by shorter or longer descriptive Greek or Latin sentences. But even in these early days there was, here and there, the beginnings of classification by obvious characters indicating varying degrees of natural relationships. In the Europe of renaissance the pulse quickened. Up to this time those who were at all concerned with plants and their utilization, being scholastically minded, could think only in terms of the ancient Greek and Latin masters. All attempted to refer their plants to those recognized and named by the classical authors, particularly Dioscorides. In northern Europe, with the invention of printing and the general advancement in learning, it became evident that many of the species characteristic of this part of the continent were really different from those of the Mediterranean region. Once this break came with classical traditions, progress was greatly accelerated, as evidenced by the masterful works of Fuchs, Brunfels, Bock, and others, for these pioneers had returned to the actual study of plants as opposed to merely a study of the classics. Following the epoch making discoveries of the pioneer Portuguese and Spanish navigators the small stream of botanical knowledge became a flood.

Still for the most part the cumbersome system of designating species by descriptive sentences prevailed and no radical change was made in nomenclature until 1753, when Linnaeus promulgated his very simple and very obvious binomial system. I say "very simple and very obvious" because it was so simple and so practicable that one constantly wonders why it was not developed as a system some centuries earlier. The idea of the genus had taken root at an earlier date, and following Linnaeus's innovation this radical departure in designating plant species by a *binomial,* a generic and a specific name, quickly prevailed. After all, in common everyday parlance the binomial system of designating plants was widely used among the common people of many countries, but there was a wide gulf between daily usage of the people and the learned world. Witness binomials in the common names of plants, such as white oak, red oak, cork oak, burr oak, live oak, scrub oak, swamp oak, post oak, chestnut oak, valley oak, holm oak, pin oak, water oak, willow oak; stone pine, sugar pine, white pine, red pine, yellow pine, nut pine, Scots pine, Austrian pine, black pine, loblolly pine, jack pine, and digger pine. This system of common names as binomials is not modern, but is one of the most ancient things in many languages, this usage being very widespread in the world at large, and among primitive as well as among culturally advanced peoples.

But coupled with the Linnaean binomial system was his artificial system of classification based essentially on the number of carpels and the number and

arrangement of the stamens. This was a very *practicable* system for arranging genera as a matter of convenience and it dominated the field for somewhat longer than the succeeding half century, although by the end of the eighteenth century the handwriting was on the wall, and in the early part of the nineteenth century the artificial system was generally replaced by the natural system of classification with which we are familiar.

If the proposal of the binomial system by Linnaeus raised a mild storm among those accustomed to the earlier much more cumbersome system of nomenclature then in vogue, a storm that quickly subsided leaving the binomial system universally established and accepted, the proposition to arrange the genera in natural families raised a veritable hurricane among the devotees of botany accustomed to the simple and convenient Linnaean system. This storm raged for some decades and we of the present age have little conception of it.

In 1831, John Torrey published his American edition of Lindley's "Introduction to the Natural System of Botany." He states in advertisement: "In France, the natural or philosophical method has for many years past taken the place of the artificial sexual system of Linnaeus, and recently by the labours of Brown, Lindley, Hooker, Greville, and others, it has begun to be employed in England and Scotland. . . . . I at once perceived that a desideratum in British and American botany, long felt and lamented, was at length supplied. It therefore occurred to me that I could not do a more acceptable service to the friends and cultivators of Botanical Science in the United States, than by preparing an American edition for the press forthwith. . . . . This is an epitome of modern philosophical Botany, and will be found highly useful to those who wish to obtain an accurate knowledge of the Natural Classification of the Vegetable Kingdom."

At this time all botanists in the United States, with the exception of Rafinesque, were professed Linnaeists; there was no other system of classification as far as they were concerned. What happened? Consider Amos Eaton's statement of 1833.[1] In speaking of Torrey's edition of Lindley he wrote:

"Since Dr. Faustus first exhibited his printed bibles in the year 1463, no book, probably, has excited such consternation and dismay as Dr. Torrey's edition of Lindley's Introduction to the Natural System of Botany. And to make the horrors of students, as well as of ordinary teachers still more appalling, Dr. Torrey's Catalogue of American Plants at the end of his Lindley, was so singularly presented, that it would seem to indicate an awful catastrophe to all previous learning. To relieve all concerned, let me make this pledge: Nothing new is presented either in the text or in the catalogue [*i.e.*, Eaton's own Manual], excepting what ought to have been discovered in this *progressive* science, since the fifth edition of this Manual was printed; and

[1] **Eaton, A.** Manual of Botany for North America, ed. 6, i-vi. 1833.

not much *real improvement* has been added, as between the fourth and fifth editions. . . . . As far as I have any influence I pledge it here, that the embarrassing innovations of De Candolle and others are *no possible use* to the science of Botany. . . . . An attempt is made in his Lindley to prove that the Artificial method of Linnaeus is unnecessary. In doing this he proposes an *Artificial Method*[2] of eleven pages. As those who have not read Torrey's Lindley will scarcely believe this unaccountable absurdity, they are requested to examine, unbiased, that work between pages lxvi and lxxx of the introduction. This artificial system [artificial key to families] is said to lead to the Natural Method. . . . . The improvements upon Linnaeus, which have been made, do not authorize any change in the science of Botany other than mere additions and corrections. . . . ."

This caustic critique of the natural system of classification is eliminated from the seventh (1836) and eighth (1840) editions of Eaton's "Manual," and in these, although he adhered to the Linnaean artificial system of classification, he so far relented as to include an epitome of the natural system. If, however, one needs a good illustration of a closed mind, here we have it, and this statement is made in all due regard to Eaton's remarkable accomplishments[3] although it is only fair to explain that in botany Eaton never claimed originality. He states[4] that in the field of botany he never aspired to be anything above that of a *teacher, translator,* and *compiler.* It should be noted that Eaton italicized his characterization of botany as a *progressive* science, yet at the same time insisted that the suggested improvements on the Linnaean system did not authorize any changes in the science of botany other than mere additions and corrections! This is an ultra-conservative, nay, even a reactionary attitude.

McAllister, p. 235, quotes from John Torrey's letter of November 2, 1833, to L. D. von Schweinitz giving his reaction to edition 6 of Eaton's "Manual": "This time Torrey was *more effusive* (italics mine) in his praise of the *Manual* when he wrote to his friend De Schweinitz 'Have you seen the 6th edn. of Eaton's Manual of Botany? . . . . I began to read the preface in a bookstore the other day & it seemed to be a most remarkable performance.' " In view of the circumstances one wonders if the term "effusive" is the corect one, for in

[2] Eaton apparently wrote this very hurriedly, for this statement regarding an artificial method is an error. What is presented is an *artificial analysis* of the orders in the form of a key to the classes (Vasculares, Cellulares), subclasses (Exogenae or dicotyledonous plants, and Endogenae or monocotyledonous plants), tribes (Angiospermae, Gymnospermae, Petaloideae and Glumceae), and to the families under each division and subdivision, these, as to limits (but naturally not as to sequence as at present understood) much the same as they stand today. Torrey's "singularly presented" catalogue is merely an arrangement of the genera of North American plants by families under the natural system!

[3] **McAllister, Ethel M.** Amos Eaton. Scientist and Educator, i-xiii, 1–587, illus. 1941.

[4] Manual. ed. 7. iv. 1936.

the same letter Torrey also says[5] that he had scarcely seen more than the covers of the book and that he was interrupted before he had finished the first page; and this first page begins with Eaton's castigation of Torrey, my quoted passage: "Since Dr. Faustus first exhibited his printed bibles in the year 1463, no book has, probably, excited such consternation and dismay as Dr. Torrey's edition of Lindley's Introduction to the Natural System of Botany." I am afraid that the dear lady didn't read this preface, for under the circumstances Torrey's statement to De Schweinitz can only be interpreted as sarcastic and ironic, as far as a gentle soul like John Torrey could be ironic and sarcastic, certainly not as "effusive" praise! The relationships between Eaton and Torrey had their ups and downs. Clearly we do not have to confine our reading to the opinions of modern botanists to learn just how certain individuals judge their contemporaries, for throughout botanical history individuals have not hesitated to say just what they thought about the work of this or that author. In the constant quibbles that one notes in taxonomic literature one is reminded of a remark ascribed to President Lowell when some acute problem regarding the interrelationships of certain prima donnas among Harvard botanists needed to be settled: "What is it about the pretty little flowers that makes the botanists quarrel so much among themselves?"

Within a decade or two from the time that Eaton castigated Torrey for his progressiveness, the Linnaean system of classification was entirely outmoded and abandoned, and was replaced by the natural system that he so violently condemned. Eaton, the non-progressive botanist is, as a botanist, only a vague memory among the devotees of this science today. But Torrey, who was the subject of his scorn, forged steadily ahead to become the outstanding American botanist of his time; and this organization, the Torrey Botanical Club, the oldest botanical association in America, today celebrating the seventy-fifth anniversary of its establishment, honors John Torrey's name, and its founders incidentally honored the organization itself, in the selection of its name, a perpetual reminder of the services rendered by this outstanding individual and botanist. Had Torrey been another Eaton, clearly there never would have been a Torrey Botanical Club.

Because of the vast number of organisms that the naturalist must deal with as to species, to say nothing of higher categories such as genera and families, it is clear that it is impossible to arrange large groups *in any lineal arrangement* that will show all natural relationships. This is particularly true of the major groups. We may follow the Bentham and Hooker system for convenience, treating in sequence first the dicotyledonous plants, then the gymnosperms, and then the monocotyledonous groups, although this is a very unnatural arrangement because the gymnosperms are infinitely more primitive, among the flow-

[5] Mem. Torr. Bot. Club 16: 280. 1921.

ering plants, than the dicotyledons and the monocotyledons. Or we may select to follow the Endlicher system as developed by Engler and Prantl, treating the gymnosperms first, then the monocotyledons and finally the dicotyledons; or we may decide with Wettstein and others, that the dicotyledons should be placed before the monocotyledons if the system is to be a natural one, in accordance with various lines of evidence as to the comparative times of development of these last two groups.

It is inevitable that when a proposed system becomes very widely used, like that of Bentham and Hooker, or that of Engler and Prantl, it will become more or less fixed, partly from the weight of authority, partly because of convenience and for comparative purposes. We may all realize that the Engler and Prantl system of arranging families, in some respects is far from a *natural* one, and that radical changes are indicated, particularly in reference to the position, in sequence, of such families as the Magnoliaceae, Ranunculaceae, Berberidaceae, etc., which seem clearly to be much more *primitive* than the Amentales, for example. System after system may be proposed, but relatively few of these will, from the very nature of things, become widely accepted as to the sequence of arrangement of major groups, partly from inertia on the part of working botanists, partly because it is always desirable to be able to make direct comparisons with the work of others, and partly because one is never sure as to just when some morphologist may discover evidence that upsets all previously proposed systems and sets up another "improved" one. It all comes down to the simple fact that within the plant kingdom, when one is dealing with such groups as natural families, it is impossible to make any *lineal* arrangement that will show all relationships and inter-relationships, for development and differentiation has not followed a straight line from a lower to a higher group, but in many cases it has been divergent, and, we may suspect, reversions have played their part. To indicate natural relationships we must construct variously branched "trees" to show origins and relationships as well as historical sequences; but in a book we must hew pretty closely to the straight line, whether we are dealing with a series of families in a system of classification, or whether we are dealing in terms of a simple manual for field use, for one page follows another from beginning to end.

Again, we must always keep in mind that the objects with which we are dealing are variable; that our accumulated knowledge constantly increases; that a system that we might set up today, on the basis of the available data, may be outmoded a few years hence when more comprehensive collections, and when a more intensive study of obscure details, perhaps supplemented by anatomic, cytogenetic, genetic, historic, and geographic data, become available. This comment applies more to the problem of species and their interrelationships than it does to larger categories such as genera and families. All

active systematists are familiar with these factors from their own daily work. As examples, I may cite my own experience. In 1904, I hopefully prepared a key to the 21 then known Philippine species of *Medinilla,* not realizing what changes would be necessary within a few years, for less than twenty years later, about 125 species of this genus had been described from or accredited to the Philippines. In 1900 there were actually known from the Philippines only 13 species of the Pandanaceae, *Freycinetia* with 7 species, and *Pandanus* with 6, of which only one was definitely understood and could be placed in reference to other described species of this genus, five described by Blanco appearing in all botanical literature as *species ignotae* or *species dubiae.* Twenty-five years later not only had all of Blanco's "unknown" species been placed, but the total for the family stood at 93 species, *Freycinetia* 45, *Pandanus* 47, and *Sararanga* 1. This is what has happened in family after family and genus after genus within the present century as comprehensive collections have been assembled from the botanically little known parts of the world such as China, the Philippines, Malaysia outside of Java and to a certain degree the Malay Peninsula, Siam, Indo-China, tropical Africa and tropical America. What is the reaction of local taxonomists, working on a restricted flora, the constituent elements of which are well known, in reference to such a work as that of Schlechter[6] in which no less than 1153 new species of orchids are described in one work, and these all from German New Guinea? The area of German New Guinea is 68,500 square miles, and for comparison that of New York State is slightly less than 50,000 square miles. Incidentally, approximately 2500 new species of orchids have been described from the Island of New Guinea since 1900. These cited examples merely represent a few that demonstrate the acceleration of what happened within the present century as various parts of the world were opened up to botanical exploration. What happened in various parts of the world happened in the United States when the West was opened up by exploration, and still later when a respectable body of local botanists developed in the West. This is, in part, the basis of the break between Asa Gray and E. L. Greene, for Greene was on the ground and was intimately acquainted with the local flora of California; I say "in part" because there was also an entirely different concept between the two as to what constituted a species.

It will be a long time yet, at our present rate of progress—which may be greatly slowed down in the coming years—before the imperfectly known regions mentioned above may be considered to be even reasonably well explored. Until this end is attained all treatments of all large groups that have representatives growing in these vast and only partly explored areas can

[6] **Schlechter, R.** Die Orchideen von Deutsch-Neu-Guinea. Repert. Sp. Nov. Beih. 1: i-lxvi. 1-1079. 1911-14.

be considered only as tentative. We do the best that we can with what we have at hand, and optimistically hope for the best. One closing example. In 1800, about 65 species of *Ficus* were more or less definitely known from the entire world. In 1801 Willdenow[7] described four new species and rather naïvely remarked: "Je ne doute pas que dans le climats chauds il n'existe encore plusieurs espèces de figuiers encore inconnues," little realizing that before the year 1940, a total of approximately 2,400 binomials would actually be proposed in this Brogningnagian genus—God forbid that these 2,400 binomials represent 2,400 distinct species, but the number of valid ones is very great, certainly approaching 2,000, even without splitting hairs on specific differences. If any taxonomist is looking for new worlds to conquer, I recommend that he undertake a monographic treatment of this vast assemblage.

In citing the above examples of the rapid increase in the numbers of proposed species in certain genera, far be it for me even to suggest that the actual naming and describing of new species is an end in itself, or if there is anything difficult about the art. As a matter of fact it is a very easy and simple matter to name and describe a species *as new;* it isn't so easy to determine whether or not the particular form in hand has been named and described by some earlier botanist or whether it actually constitutes a sufficiently distinct entity to be considered worthy of consideration as a species; to say nothing about macrospecies or microspecies, nor even to mention subspecies, variety, subvariety, form, proles, or any other category that has been suggested, but never too well defined, to indicate minor entities. With the myriads of forms with which we must deal we must have names. The competent monographer follows and either embalms our possible error by recognizing a species as valid, or sinks it into synonymy; and if the latter happens then at some future date some other monographer may reinstate it with the chances that in the interim some other optimistic taxonomist may have renamed and redescribed the same form under a new name in his confidence that a published reduction is always a reduction, which, perhaps unfortunately, is not always the case.

The special properties of a very high percentage of our thousands of species of economic plants, whether utilized for food, for medicine, for fibers or for any other purposes were originally discovered by empirical processes and by observation rather than by direct and deliberate investigations. This is the history of most plant species of economic importance whether it be the lowly bean used for food, or the insignificant looking *Ephedra sinica* now extensively utilized in the practice of medicine. Although this *Ephedra* has been utilized by the Chinese for many centuries it is only within the present century that it was definitely demonstrated that its curative principle ephedrine is really of

[7] **Willdenow, C. L.** Determination de quelques nouvelles espèces de Figuier, et observations générales sur ce genre. Mem. Acad. Sci. [Berlin] **1801**: 91–104. *t.* 2–5. 1801.

distinct value in the treatment of asthma and various diseases of the nasal passages. Through taxonomy, however, a realization of the relationships of plants, we find what may be an important lead. If *Ephedra sinica* yields ephedrine, isn't it possible or even probable that other species of the same genus may yield the same curative agent? Thus a pharmacological investigation of all species of *Ephedra* might be indicated, for the sole natural source of *Ephedra sinica* is northern China, although other species of the genus occur in various parts of Asia, Europe, and North America. It is admitted, now that ephedrine has been synthesized, that further work on representatives of this particular genus may scarcely be worthwhile, but the case serves to illustrate the problem of botanical analogy.

Take the case of chaulmoogra oil, now extensively and successfully used in the treatment of leprosy. For centuries this oil was used in India for the treatment of leprosy and various skin diseases. For nearly a hundred years the situation was confused because the plant named by Roxburgh as *Chaulmoogra odorata* Roxb., but never actually described by him, was supposed to be the species that yielded the effective drug; yet the seeds of Roxburgh's species, later described as *Gynocardia odorata* R. Br., when investigated, were shown to contain no active curative principle. It was not until 1900 that Sir George Watt cleared up the confusion and determined the botanical source of the true chaulmoogra seeds as *Taraktogenos Kurzii* King = *Hydnocarpus Kurzii* Warb. Rock,[8] who has discussed this subject, states that it is quite probable that not only seeds of this species but also those of *H. castaneus* Hook. f. & Th. and other species of *Taraktogenos* and *Hydnocarpus*, as yet undescribed, are sources of the chaulmoogra oil of commerce. The botanical confusion that prevailed for a hundred years unquestionably retarded a critical and serious investigation of chaulmoogra oil as a remedy for leprosy. It is only within the present century that this cure has come into its own.

Intrigued by the problem of analogy and suspecting that the seeds of some of the Philippine species of *Hydnocarpus* might contain the same curative principles as the true chaulmoogra oil, I was instrumental in fostering an investigation of those Philippine species that were available, including *Hydnocarpus Alcalae C. DC., H. subfalcata* Merr., *H. Woodii* Merr., and *H. Hutchinsonii* Merr. Various studies were made in the Bureau of Science culminating in 1928, when Messrs. Perkins and Cruz[9] investigated the oils of ten species including four from the Philippines and Borneo, and found that in these four species the oil was very similar in chemical composition to commercial chaul-

[8] **Rock, J. F.** The Chaulmoogra tree and some related species: A survey conducted in Siam, Burma, Assam, and Bengal. U. S. Dept. Agr. Bull. **1057**: 1–29. *t. 1–16.* 1922.

[9] **Perkins, G. A.** and **Cruz, A. O.** A comparative analytical study of various oils in the chaulmoogra group. Philip. Jour. Sci. **23**: 543–569. *t. 1.* 1928.

moogra oil except that *Hydnocarpus Alcalae C. DC.* contains a very large amount of chaulmoogric acid and little or no hydnocarpic acid. The total percentage of oil varied from a minimum of 11 percent to a maximum of 39 percent. Now as far as known none of the Philippine and Bornean species was utilized for any purposes by the native population. They were, of course, unknown to the small technical public outside of the very few botanists, and it is an interesting commentary to note that as to the Bornean *Hydnocarpus Woodii* Merr. trees were actually found to be growing within the limits of the leper colony on Sandakan Harbor; a remedy actually at hand, but previously unknown, and its potentialities hence unrealized.

In the latest treatment of this group[10] *Taraktogenos* Kurz is reduced to *Hydnocarpus* Gaertn. and a total of forty species are recognized. Not more than one-fourth of these species have been investigated from a pharmaceutical standpoint; and yet from what is known of the properties of those that have been investigated it is safe to assume that the seeds of most of the species of the genus will be found to yield the same curative principles as are found in the true chaulmoogra oil.

Thus from analogy, working from a Burmese species, the curative principles in its seeds being known, investigations extend to the seeds of the Philippine and Bornean species of the same genus, *Hydnocarpus,* with potentially important economic results. These examples will suffice to demonstrate what has been done in special cases, and by analogy we may expect that in the future similar investigations will be extended to very many species that have hitherto never been considered as even worthy of investigation; but in a reasonable percentage of cases we may definitely assume that these species, as yet unknown and unappreciated from an economic standpoint, will be shown to produce needed and otherwise unattainable products. Here the tempo increases under the pressure of necessity brought about by war conditions in reference to supplies of rubber, quinine, and various other products for which, in the past, we have depended largely on Asia and Malaysia for our supply; and our economy and even way of life was increasingly geared to various imported basic products which now are unobtainable elsewhere. Now new sources must be developed, if not from the same species so successfully developed in the specialized agriculture of certain parts of the Old World (even although in some cases based on native American plants, such as *Hevea* and *Cinchona*), then from others that yield similar products. It is in this specialized field of potential substitute plants that may yield important products that we now lack, that the trained and experienced taxonomist can render, and is rendering, fundamentally

[10] **Sleumer, H.** Monographic der Gattung *Hydnocarpus* Gaertner nebst Beschreibung und Anatomie der Früchte und Samen ihrer pharmakognostisch wichtigen Arten (Chaulmugra). Bot. Jahrb. **69**: 1–94. *t. 1–4.* 1938.

basic services. It is this type of individual who knows his plants and who knows plant relationships who can serve to great advantage, for his accumulated store of special knowledge cannot be matched by those botanists trained and experienced in other fields remote from that of taxonomy and systematic botany. Let us hope that those charged with selection for super-specialized services such as those indicated in this field of botanical analogy, will select wisely and well. After all there is much truth in the popular conception of what a botanist is—an individual who knows and can name plants; yet the vastly higher percentage of our professional botanists have almost no knowledge and less experience in this specialized field of taxonomy, and many of them have no interest in it. They are for the most part specialists in totally different branches under the all-inclusive term botany, for in our times the term botanist covers not only the taxonomist and systematist, but also the fields of morphology, physiology, ecology, cyto-genetics, cytology, histology and various other subdivisions; the numerous devotees to these subdivisions of botany are all "botanists" in spite of the popular definition cited above. A very high percentage of them would be utterly lost were they to be assigned to special problems in this distinctly complicated field of botanical analogy.

Within the field of medicine or pharmacology, here is a simple illustrative case. The European *Digitalis purpurea* Linn. is the source of an important drug, digitalin, and we have generally depended on Europe for our supply. With these supplies now cut off by the war, local sources must be developed. I have no idea of how extensively the plant is now cultivated in the northern United States, but Fernald, on the basis of his own extensive field knowledge, calls attention to the fact that the species is not only thoroughly established in certain parts of Newfoundland, but that in places it is dominant and a veritable pest; a source of supply that only needs to be tapped if there be need to build up our dwindling stocks, and an indication that certain parts of Newfoundland are ideally adapted to the actual cultivation of the species on a large scale if this be needed.

It is clear to all taxonomists and all systematic botanists, that in spite of the imperfections in our current system of naming and describing plant species, and in spite of the distinctly Rafinesquian character of the work of certain individual botanists who can see differences where tangible differences scarcely exist, that taxonomy and the accurate identification of plants is basic to a proper understanding of myriads of problems in the general field of economic botany, pharmacology, agriculture, plant breeding, plant pathology, genetics, forestry, morphology, physiology, and many other fields into which plant science or botany *sensu latiore* has been subdivided. We have little patience with the investigator, no matter what his problem may be, who ignores this basic problem of accurate identification of the material with which he

deals. Obviously if one deals with misidentified material his findings may prove to be valueless, for future investigators will find it difficult if not impossible to check his results. There are too many errors in botanical literature due to this lack of critical consideration of this simple basic problem, and much time, and some space in our technical periodicals, has been wasted due to the ignorance or the blind faith of investigators, or those who have stimulated research on a particular subject, who have not considered it to be either essential or even worthwhile to check, or to have some competent taxonomist check, the identity of the plant utilized to prove this or that conclusion. Here is a horrible example:

In 1902 there was published in one of our leading botanical magazines a paper on the morphology of the flower and embryo of *Spiraea* that admirably illustrates the importance of accurate identification. The investigator worked with material representing a single species, the plant widely known among horticulturists under the erroneous name of *"Spiraea japonica."* Far from being a representative of *Spiraea* or even of the family Rosaceae this plant is *Astilbe japonica* A. Gray of the Saxifragaceae. The author completed his detailed study without even suspecting that he was dealing with a misidentified plant, from which we may assume that he could not have done much bibliographic research as the differences between *Astilbe* and *Spiraea* are remarkable. Is this blind faith in a labelled growing specimen or sheer carelessness or ignorance on the part of those who suggested and supervised the work and thus victimized an innocent graduate student who had faith in the knowledge of his preceptors? The net result was to discredit the student, for about all he got out of it was some training and experience in laboratory technique, discredit to the periodical in which the article appeared, and, may we hope, some discredit on those who sponsored the investigation. It is a classical example of how not to elucidate a morphological problem, for the net result merely served to stimulate the glee of the lowly taxonomists who, as a group, are thoroughly satiated with the "holier than thou" attitude of some of our colleagues in the laboratory aspects of botany. I am much less charitable than was Rehder who called attention to the error.

What do we taxonomists think, when we observe in a physiological paper a tabulation of species whose seeds will not germinate until after they are subjected to freezing temperatures and note the strictly tropical *Carica Papaya* listed in this category? True, pawpaw and papaya are common names of *Carica Papaya* but pawpaw is also the common name of our entirely different northern *Asimina triloba* Dun. We can only assume that the seeds of *Asimina* were what this investigator had, for *Carica* is a plant entirely intolerant to freezing conditions. All of which merely illustrates that we should not put our trust wholly in the currently used common names of plants. After all, "What is

in a name, a rose by any other name would smell as sweet" but in cases like these, one is reminded of an expression used by one of the characters in that intriguing comedy, "You can't take it with you" when he was expressing his opinion of the dancing ability of another character in the play.

In this part of the discussion I am rapidly approaching a category recently discussed in the daily press. Under date of May 18, it is reported from Raleigh, North Carolina, that some years ago the Daughters of the American Revolution planted, with elaborate ceremony, a little tree purported to be an offspring of the "Continental Elm" at Cambridge, Massachusetts, under which George Washington is supposed to have taken command of the Continental Army in 1775. They even kept a box of earth taken from around the roots of the parent tree for use in christening the "elm" when it grew up. The little "elm" has grown up and is now blooming; but it is a cherry tree and not an elm at all. Assuming that the young tree that was planted was provided by some nurseryman this merely proves that nurserymen and horticulturists can make mistakes just as botanists do, but is this any reason why a botanist making a really serious study of a plant problem should accept without question as to its correctness, a commonly used but erroneous horticultural name, or should determine what binomial he should use merely by looking up a common name?

One closing example, that of the investigator who had laboriously dug up and intensively studied the root tips of *Tilia* in one of our large collections, and could not understand the discrepancies between the chromosome counts of the root tips and of the branchlets taken from the same trees in a number of cases. It was only after the study had been completed, but fortunately not published, that he learned that many of these species of *Tilia* were grafted, the roots representing an entirely different species from the growing tree. Thus for certain types of investigations we cannot even trust the living plants without knowing something about their history.

I have above referred to the fact that during the many centuries Europe was dependent on its own economy, its inhabitants utilized only a relatively few plant species; a few hundred important ones at most. As various parts of the world were opened up within the few centuries following the expansion of the European colonizing nations the number of species utilized rapidly increased; and this tempo of increase continues unabated. In 1853, Linnaeus recognized 5,950 species of plants in all groups for the entire world, while he and his immediate followers estimated that there might be as many as 10,000 species of plants, in all groups, in the world. The estimate had been increased to 30,000 known species by 1820, and 50,000 indicated as probable for the entire world. By the middle of the century the estimate of known species was 93,000.

Within the present century about 265,000 new binomials have been pub-

lished for the flowering plants and vascular cryptogams alone, of which about 194,000 represent hopefully proposed new species, the remainder shuffles or transfers from one generic name to another. The yearly average for the higher groups alone is now approximately 6,500 *as new binomials,* of which about 4,750 represent proposed new species. This is the record of the twentieth century to date. The total number of binomials published from 1753 to 1942 is in the neighborhood of 750,000 for the higher groups of plants alone, and to this must be added those published for the cellular cryptogams; our grand total should be in excess of 1,000,000.

As to the total number of distinct and more or less "known" species, who shall say? Jones has briefly discussed this matter[11] calling attention to the remarkable discrepancies that occur in recent texts, with a spread in the estimates of from 133,000 (Uphof's estimate of 1910) to 175,000 for the angiosperms alone, and concludes that the total for all known groups is in the neighborhood of 335,000. Because of various complications that it is unnecessary to discuss here, I suppose that we may conclude that one guess is as good as another; but knowing something about synonymy; something about the limiting factors in the geographic distribution of individual species; something about more or less universally distributed species; something about the extraordinary richness of tropical floras; something about the remarkable local endemism in various tropical areas; something about the high percentage of novelties that are found in all new collections from hitherto inadequately explored areas; something about those regions that, within the past four decades, have been particularly rich in the crop of new species—my guess is pretty close to that of Jones, and that the total number of reasonably valid described species in all groups is well in excess of 300,000. Even if the number of valid species should be only half this total, what scientist, no matter what his field, would even have the temerity to suggest that we can get along without taxonomy and nomenclature?

In this discussion I have deliberately been discursive rather than specific. One could cite case after case of the applications of taxonomy to various scientific and economic problems, but a few will serve to bring out the points at issue. Besides those mentioned above in my discussion of botanical analogies we may list the problem of the *Citrus* relatives; the case of *Coffea arabic*a Linn. versus *Hemileia vastatrix* Berk.; *Berberis* versus wheat rust; the *Pinus-Ribes* complex in reference to the blister rust of the white pines; the little problem of special strains in such lowly organisms as the yeasts and the fungi when these organisms are basic to certain industrial processes—the list would be unending, for no agricultural crop exists in which problems of plant breeding, of protection against fungus diseases and insect pests do not exist. Many prob-

[11] Jones, G. Science II. 84: 243. 1941.

lems have been solved, but many more are still with us, and new ones develop from year to year. With all due regard to the qualifications and accomplishments of the specialists in the various fields concerned, I maintain that the better equipped the investigator is in basic taxonomic knowledge, the better is he fitted to work on his special problems. This does not mean that all botanists should be taxonomists, but it does mean that all specialists and all laboratory botanists should realize the importance of accurate identification, the implication of botanical analogies, and that they should appreciate the facilities outside of their own fields that are available in specialized institutions in various parts of the country. We will go much further with reasonable cooperation than we will by maintaining a pigeon-hole type of specialization.

There should be no real antagonisms between the devotees of various aspects of botanical science, for the inter-relationships are close—much closer than some of our specialists realize. We are all laborers in the same vineyard, and our objective is progress; progress in pure science as well as in the economic aspects of the subject as a whole. To those representatives of the laboratory school of botany who are hypercritical regarding taxonomists and systematists, I would call attention to the fact that progressive taxonomists are now taking advantage of the findings of their associates in other fields including the histologists, pollen experts, geneticists, cytologists, ecologists, and entirely outside of the biological field invoking the aid of geologists, hydrographers, geographers and others in their attempt to solve certain problems of plant relationships.

This very organization that this week celebrates the seventy-fifth anniversary of its establishment was founded by individuals whose fields of interest were essentially field botany, taxonomy and systematics. It has evolved, during the course of years into a national organization and has wisely and progressively widened its activities, yet the unifying idea that maintains it is still that of its founders who were interested in plants and who knew plants as they grew in nature rather than merely as laboratory subjects. I repeat what I have written before: "It has been fashionable in some quarters in modern times to decry both the importance and the value of systematic botany. Because of its vitality, its human interest, its practical bearing on other phases of plant science, and on our everyday life, one suspects that some of its critics have lacked the breadth of view of leaders in science, and have been misguided in criticizing that which they did not fully understand."

Let us take the broader view, live and let live, keep our respective houses in order, avoid egregious blunders, and attain a realization of the fact that after all there is a unity in plant science in spite of its diversity, and that the entire field is interlaced with the binding bonds of system and order; and this is taxonomy.

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